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Hydrology and Earth System Sciences Discussions

Interactive comment on "Multi-objective automatic calibration of hydrodynamic models utilizing inundation maps and gauge data" *by* N. V. Dung et al.

Anonymous Referee #3

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This paper deals with the use of gauged level records and ENVISAT SAR images to support and verify flood inundation modelling. Particularly, the paper investigates the use of SAR images and gauged level record for the multi-objective automatic calibration of a flood inundation model. The idea presented is definitively interesting and the paper is well written (only few parts requires to be corrected in terms of use of English language) and structured. By the way some aspects of the methodology require further clarifications research and some points should be corrected. Specifically, the following speciïňĄc points need to be addressed:

1. page 9180, section 3, line 11: How were the ïňĆood extent maps obtained? The authors should brieïňĆy describe how the SAR images have been processed.

2. page 9180, section 3. More detail about the topography of the area should be given. For example, how large is the model domain and which is DEM horizontal resolution? But, it is not clear to me why the 1D model was chosen for this study. Estuarine areas are the perfect test case for 2D models giving their particular topography, i.e. essentially flat without a well defined river bed and river sections. Correctly, the authors refers to the original Mekong Delta Model, a quasi 2D model (channel and weirs) but here the 2D effects are modelled using a probabilistic-fuzzy approach which, in my opinion, introduces high uncertainties giving its "subjective" nature. Why not simply use the DEM to create a 2D inundation information using the water level modelled in the channel and the flood volume with a simple interpolation on the inundated cells?

3. page 9182, line 2. Please expand "resp."

4. page 9182, section 4. I think some considerations on the ranges of roughness parameters considered should be done. In my opinion the values within the ranges are not consistent with the physical characteristics of the area, especially for the lowest values (10-20 for Strickler which means "natural stony streams" or "dense vegetated riverbed" or "urbanised areas" (see Fabio et al, 2009). Now I'm not sure the Mekong delta can be assimilated to a steep stony mountainous streams being essentially a flat area where water flows are quite slow. This assumptions (together with the use of F2 OF which is biased towards large inundation extent, i.e. its value increases with larger inundation and thus may lead to unidentiïňĄable parameter spaces) can return very low calibrated values (Table 6) which doesn't sound to me. I suggest to perform a Monte Carlo sensitivity analysis to explore the parameter space and to justify in a less subjective way the choice of roughness coefficient ranges.

5. Section 4.3.1. The authors should supply some details on the way the weights in the F1 have been chosen. In my opinion this choice can have a large influence on the final

results of the calibrations and, probably, the “strange” values of roughness coefficient obtained can be explained by this. As above, I suggest to perform a sensitivity analysis in order to quantify the influence of these weights on the final results.

6. page 9189, equation (5). As above in #5 but for F2.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 9173, 2010.